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THE SIGNIFICANCE AND RELATIONSHIPS OF THE THOMAS HENRY SIMPSON MEMORIAL INSTITUTE FOR MEDICAL RESEARCH[1]

TO-DAY you have opened a research institute, dedicated to the study of an important and serious disease. May I not present to you some thoughts as to the significance and relationships of such an institute?

But first let me congratulate the faculty of medicine and the medical students of the University of Michigan on the possession of the Thomas Henry Simpson Memorial Institute for Medical Research, and the donor on the wisdom evinced by the form of her gift as a memorial to her late husband. To you, Mrs. Simpson, would I say that I feel certain that this gift will prove a source of real pleasure and great pride to you as you follow its work. However, the span of your life, and we trust it may be long, will be but a brief space of time in the life of the institution you have founded; as a memorial it will stand and be a productive institution for unnumbered years to come. Few, if any things, are more permanent than institutions of learning and hospitals. In history they have survived dynasties and peoples; their forms may change, but the spirit lives on; buildings crumble and fall, but the idea, embodied in the foundation, is immortal. So we may anticipate that, opening to-day, the Thomas Henry Simpson Memorial Institute for Medical Research will in some form last as long as does our civilization or a superior one.

Through all the ages there has been some form of quest to fire the zeal of man. In the days of chivalry such a quest was typified by the search for the Holy Grail,

> "That so perchance the vision may be seen
> By thee and those, and all the world be heal'd."

A little later and the quest took the form of exploration and settlement of new lands. Now, with unexplored lands almost non-existent, the quest has shifted to investigation, the search for truth and the discovery of new facts. Investigation has become the Holy Grail of science, and the quest in this form stirs the imagination and fires the zeal of a new type of Sir Galahad, to

[1] An address delivered at the opening of the Institute, University of Michigan, February 10, 1927.

"Go forth, for thou shalt see what I have seen,
And break thro all, till one will crown thee king
Far in the spiritual city."

Pure, as typified by the search for the Holy Grail, is the purpose of this new institute for medical research. The results of work, done within these walls, will be given freely and without price to suffering humanity. As the Holy Grail was seen, but never attained by the knights of old, so the purpose of this institution will never be attained in the sense that its work can be said to have been finished. If within these walls the cause of pernicious anemia is discovered, and it is our hope that this may be the great good fortune of the Thomas Henry Simpson Memorial Institute for Medical Research, merely a larger vista for exploration will be opened. With cause known, prevention and cure more intelligently may be sought. If these are found, the scope of work at once enlarges to apply the knowledge, obtained in the study of pernicious anemia, to other related diseases that take their yearly toll of life and are causes of suffering and disability to human beings.

It is to be remembered that the by-products of investigation of a given disease often outweigh in importance and significance the solution of the main problem. Failure to solve the problem set is not a failure in the usefulness of the institute. Laws of fundamental importance to science may be discovered, whose value transcends any discovery connected with pernicious anemia. Methods, applicable in many relationships, may be evolved. Month by month and year by year young men are being trained and moulded by the struggle to solve the problems under investigation; these in later years become investigators prepared to solve other problems. All research institutes are educational institutions in the larger sense of the word.

This is not an isolated institution. Wisely it is part of a great university, intimately associated with a medical school of splendid traditions, in close juxtaposition and closely interdigitated in many ways with a commodious, well-organized hospital. This association is mutually helpful to all brought into this circle of contact; investigators, teachers, students, patients. Without such relationships the productivity of this research institute automatically would have been vastly curtailed. The related institutions furnish a soil in which the Thomas Henry Simpson Memorial Institute for Medical Research will flourish and have its fruition, and to this soil will pass out much that will greatly enrich it for other products. Progress, made anywhere within this group, starts an advance that will widen as the wave started in a still pond widens, and, like such a wave, which, as it touches a solid shore, is reflected back to initiate other waves, will spread its influence beyond the circle of immediate contacts. Nor is such an influence long limited to the environs of this university. Methods of study, ways of treatment, perfected here, will, after a very brief interval, be tested elsewhere, and, if found of value, soon all the world will be using them, for science and the medical art know no geographical bounds of country or continent.

There is a side to a clinical research institute that distinguishes it from all other institutions of research. This concerns the relationship to patients, a relationship that must ever be in the forefront of the thoughts of the investigators in a clinical institute. In any properly organized clinical institute, though the problem under study may fail of solution, constantly a benefit is being brought to the patients within the institution. It is inconceivable that these patients should not have better care and their suffering be more ameliorated than similar patients anywhere else. That they should is now recognized as the first duty of every institute for clinical research. Fortunate then will be the patients that may come to this institute for study.

Patients guarantee such a research institute's being productive of good, even though the cause and cure of pernicious anemia are never found. At the same time their presence places limitations on what may be done. The director is first of all responsible for the welfare of these patients. Nothing can be tried that is likely to injure them; nothing must be done without their free consent. In every hospital, and a clinical research institute is a hospital, the best possible care of the patients is the first duty of the institution, and all else is secondary to this. In many experiments, desired combinations of conditions can be produced by the experimenter; with patients he must await the finding of the desired combination, as it has occurred independently of experiment. In this wise clinical research may be infinitely slower than other forms of investigation. To find the desired combination, great numbers of patients must be studied. For this the affiliated large university hospital is of the utmost importance and there must be no lack of cordial, prompt and complete cooperation between the two.

Any spirit of jealousy or antagonism between these related institutions will be fatal to progress in the investigations for which this institute has been founded. If such develop, they must be eradicated at once. A very considerable experience, as head of a medical clinic, has convinced me that men who allow personal jealousies and animosities to develop are so inherently small and narrow, so prejudicial to efficiency in team work, that, whatever their individual qualifications, the institution progresses best when

they sever connections with it. A large spirit of tolerance for native faults in others is needed by all to make for that happy community of interest that seems essential for the success of any institution. An institute for research should have no interest in claims of priority for ideas or discoveries; in fact, to my mind nothing is more futile than the not infrequent discussions as to priority of discovery that we hear; time always settles justly such questions, and discussion with claims and counter-claims but delay the fair verdict of time. Eventually each investigator is credited with his own small part in the discovery, and as a rule his credit is more than his deserts, for many contribute to all discoveries. Any problem worthy of investigation is large enough to give place for more than one investigator, even though each begins with the same ideas and uses the same methods. The genius who in isolation can solve important problems exists, but they are extremely rare. That an individual believes that he is such and should have special privileges of being disagreeable to his fellows is poor proof that he is such a rare genius and little reason for tolerating his eccentricities.

To those of you responsible in an administrative sense for this institute, let me say that wisely you should leave to the director, selected by you, all freedom in determining the policies and plans of the work within the terms of the gift. In selecting him you have given expression to confidence in his ability and his character. That you have not erred, I feel sure, by reason of a long and peculiarly intimate association with him, one that it gives me great regret to end. But I am glad for the great opportunity he is to have here at the University of Michigan. To make of this institute a success, to cooperate wisely in the development of the department of medicine of the medical school and of the University of Michigan is truly a great opportunity.

Pernicious anemia, for whose study this institute has been founded, is a disease well suited to investigation. First, it is a chronic disease and of chronic diseases we know far less of cause and cure than of acute disease, because they offer far greater difficulties in investigation than do acute diseases. Second, it is a disease which has a natural cycle of betterment and relapse, suggestive of a periodic recurring cause or a cycle in which cause first acquires ascendancy and then the human organism gains the upper hand; it is not a progressively degenerative process, as are so many of the chronic diseases, and this lends probability to great improvement in methods of treatment. Third, there is much to suggest an underlying condition, without which the disease can not develop; this suggests a strong possibility that it may be amenable to prevention whenever either the cause of the underlying condition, or the cause of the anemia itself may be discovered. Fourth, already encouraging results have come from certain dietary methods, recently applied in the treatment of the disease, and there are many problems, suggested by these dietary successes, open for immediate attack. Fifth, there are with pernicious anemia extremely interesting climatic relationships, which needs must have important bearing on cause, prevention and cure, if these relationships are investigated. Sixth, one of the underlying conditions seems to play a significant rôle in a large group of diseases, suggesting an immediate applicability of knowledge, gained from the study of pernicious anemia, to a larger group of diseases which taken together are of vast hygienic and economic importance to the human race. Seventh, with many recent improvements in methods of investigation of problems of disease, the time is ripe for such a study, as is made possible by the establishment and endowment of this institute. Eighth, this is, so far as I know, the first institute founded for the particular investigation of pernicious anemia; it has no competitors. Finally, the problem, though there are many obvious points of attack, is not going to be easy of solution. Patient persistence, imagination, ingenuity, knowledge of methods, broad understanding of many diseases and their treatment, coordinated, cooperative labor, will be required. The difficulties will stimulate the best endeavor of those selected to work within this institute. To solve successfully the problems set by pernicious anemia will be a worthy reward of high endeavor. The difficulties in the way will serve to attract a very superior type of investigator.

Auspiciously the Thomas Henry Simpson Memorial Institute for Medical Research is beginning its career. Well-wishers over the country will follow its activities with interest. In its successes they will rejoice, as you, its workers, follow the trail of investigation, which may be paraphrased in the words of Tennyson,

> "Not of the sunlight,
> Not of the moonlight,
> Not of the starlight:
> O young mariner,
> Down to the haven,
> Call your companions,
> Launch your vessel
> And crowd your canvas,
> And, ere it vanishes
> Over the margin,
> After it, follow it,
> Follow the gleam."

Henry A. Christian

Peter Bent Brigham Hospital,
Boston, Massachusetts

THE FORTUNATE FAILURE OF PHILOSOPHY

Harper's Magazine for December, 1926, contains an enlivening lament over the "Failure of Philosophy," by Will Durant. In spite of his frank recognition of the unverifiable quality of philosophical speculations—a quality that did not interfere at all with the acceptance of such speculations as the basis of belief in an unscientific age—the author closes his article with a wish that philosophy might be restored to "her ancient scope and power." He therefore does not seem to recognize the inappropriateness of philosophical speculations as a basis for belief in a scientific age; in other words, he does not seem to see that what he calls "the failure of philosophy" in our present era is as fortunate and as profitable as it is inevitable. Of course there are still and long will be a good number of scientifically undisciplined persons who take their preferred speculations for the truth; if any solitary inquirer wishes to consort with persons of that class, he will still find plenty of company. But the greater part of Durant's article does not read as if *he* would find satisfaction in company of that kind.

He recalls the decline of philosophy "from the great days when she took all knowledge for her province and threw herself . . . into the forefront of the mind's advance." In that early time she "was the proud mistress of all the intellectual globe . . . Now . . . she stands by the wayside desolate, and none so poor to do her reverence. The sudden uprising of the sciences has stolen from her, one by one, her ancient spacious realms . . . Nothing remains to her, except the arid wastes of metaphysics, and the childish puzzles of epistemology, and the academic disputes of an ethics that has lost all influence on mankind. Even these wastes will be taken from her," as new sciences enter and possess them, and make them fruitful.

It is curious that a writer who sees all that so clearly does not see also that the change which he laments is merely a part of the evolutionary and profitable progress that has been made in the world of thought. Speculative and unverifiable philosophy very naturally threw herself into the "forefront of the mind's advance" in early times, because the verifiable sciences and more particularly the logical way of thinking about those sciences had not been developed. Unverifiable speculation was then well enough, but it is not satisfying to-day, because, as Durant elsewhere and very truly says, "the great events in the history of thought are the improvements men have made in their methods of thinking and research." Of course, speculative hypotheses must still be invented not only in philosophy but in science also; but in science such hypotheses serve only as first steps in the effort to reach reasonably verified conclusions concerning things and conditions and processes that transcend direct observation. Unless the process of verification, to which the ancient philosophers lent so little attention, is successful enough to give invented hypotheses a fair standing, the trained scientist to-day ranks them at their true and low value; that is, as nothing more than figments of the imagination which may or may not correspond to external verities, and which therefore do not deserve, as long as they remain unverified, to serve as the basis of belief regarding such verities.

But Durant does not seem willing to follow this course. He wishes philosophy to go beyond the verifiable conclusions of science and to "make vaster hypotheses about ultimate problems on which no verifiable data are at hand. It is a perilous completion of knowledge. It fills out with experimentally unprovable assumptions the gaps in our scientific knowledge of the world." This is as if a topographer, after mapping all the land areas that lie within reach of his observation, should not only draw in the features of unobserved areas to suit his fancy, but should himself believe and ask others also to believe that those fancied features deserve to rank along with the observed features as truthfully representing the face of the earth. Of course, if any one wishes to make perilous excursions beyond observation and verification, he is free to do so; and if he sets forth the results of his excursions in a plausible manner, he will likely enough find some persons so credulous as to accept his results as verities; but it is surely to the credit of the more disciplined students of the modern world that they demand something more than the mere invention of unprovable assumptions as a means of filling out the gaps in reasonably ascertained scientific knowledge. There is, indeed, no more striking characteristic of our intellectual advance than the growing demand for reasonable grounds for our beliefs, coupled with a frank and patient withholding of belief until valid grounds for it are found. Yet Durant seems to think that philosophy, even though or perhaps because it adopts "unprovable assumptions," is a sort of superscience; for he goes on to say: "Science is only the analytic description of parts; philosophy is the synthetic interpretation of the whole . . . The sciences are the windows through which philosophy sees the world"; and in looking through these windows philosophy does not see with "mere knowledge," but with the strongly different quality called "wisdom."

It may be questioned whether scientists will be satisfied with that self-complacent statement of the case.

To be sure, there are very likely some scientists who are merely learned and not wise; but there are also some—one has only to think of such men as Faraday, Darwin and Pasteur—who are wise as well as learned, and who show their wisdom not only in making synthetic interpretations of accumulated knowledge as far as seems reasonable to them, but also in not deceiving themselves by thinking that the unproved hypotheses which they or others may invent about ultimate problems deserve acceptance along with reasonably verified knowledge. And on the other hand, while there are some philosophers who are so wise as to recognize their unprovable assumptions as nothing more than mental concepts, which therefore have no ascertained relation to external verities—indeed, some of the Greek philosophers reached this stage of advance twenty centuries ago—there are, it would seem, others less wise who become so fond of their assumptions that they persuade themselves and try to persuade others also that the assumptions really do fill gaps in scientific knowledge.

No sharp line can be drawn between well-verified scientific conclusions and wholly unverified hypotheses. There are all grades of verification. The proof of the regular rotation of the moon on its axis while it moves at varying velocity around its orbit, as given by lunar libration, may be instanced as an example of complete verification. There is no comparable proof of ancient Gondwanaland between India and Africa, although the former existence of that now vanished continental area is made fairly probable by a large body of consistent geological evidence. As to the recently launched Wegnerian concept of the flotation and shifting of continents, the evidence is so uncertain that many geologists find no value in it. Unproved or unprovable hypotheses are therefore by no means the possession of philosophy alone; but philosophers would seem, according to what Durant says of them, to have the unenviable habit of thinking that they can really fill out the blank spaces in scientific exploration by the invention of "experimentally unprovable assumptions." The scientific estimate of that habit is surely that it is a bad one; and hence that the world is fortunate now that even philosophers are coming to see that philosophy, *as thus constituted,* is failing.

Durant hopes that, in spite of its failure in this respect, philosophy may still include the studious pursuit of several special subjects, such as logic, esthetics, metaphysics, ethics and religion. But it may be well contended that several of these subjects had better be taken over by the modern sciences to which they are related. It is only by a traditional and arbitrary assignment that the strictly disciplinary study of logic is associated with so speculative a subject as philosophy; it would be much more appropriately grouped with the mathematical and physical sciences, where it might be practically taught by the case method so that no formal or systematic course in logic would be needed. Esthetics and metaphysics may be well left to philosophy, although the discovery of "the final and real nature of matter," which metaphysics may perhaps claim as its very own affair, had to-day much better be given over to physics, where progress toward that discovery is advancing marvelously. Ethics may be safely redeemed from the "academic disputes" which have robbed it of "all influence on mankind," by making it an inductive and therefore a truly scientific study of the natural history of goodness; and religion may be similarly treated, to its great advantage. Thus limited chiefly to its self-selected task of making "vaster hypotheses about ultimate problems," philosophy would be to-day by no means "the proud mistress of all the intellectual globe," as it was to Socrates, when he advised that it should be examined well and truly, and followed and served faithfully; it is by no means clear that he would advise us to follow it faithfully if its chief task is to make "experimentally unprovable assumptions." Indeed, if Socrates were now born again, we may well imagine that, in view of his strong belief in the value of experience, he would be less a teacher of the idealistic Plato than a follower of the experiential Avenarius. In any case, wise as Socrates was in his time, our time is so utterly different from his that his leadership even in philosophy is now long and far outgrown.

W. M. Davis

Cambridge, Mass.

EDWIN THEODORE DUMBLE

Edwin Theodore Dumble was born in Madison, Indiana, March 28, 1852, and died at Nice, France, January 25, 1927. He was a student at Washington and Lee University from 1866 to 1869 and from 1872 to 1874, receiving the degree of Sc.D. from that institution a few years since. He was state geologist of Texas from 1888 to 1896 and consulting geologist of the Southern Pacific Company and subsidiaries from 1897 until the time of his retirement in 1925.

Mr. Dumble published some seventy scientific papers during the thirty-seven years of his activity as a geologist. These papers embrace a wide range of geologic subjects. His output is remarkable since during the entire time he was heavily burdened with administration work. Although perhaps the most successful of all economic geologists, his main interest was pure geology. His predilection was for the somewhat disheartening field of the Texas Tertiary.

As state geologist of Texas he gathered about him an assemblage of truly remarkable men. The group includes Penrose, Taff, Tarr, Harris, Kennedy, Cummins, Hill, Osann, Von Steeruwitz, Drake and Owen. Dumble and these men in five years' time erected the enduring substructure of the geologic knowledge of more than one twelfth of the total area of the United States. Their achievement is in every sense equal to the pioneer work of Hayden, Gilbert, Powell, Dutton, King, Emmons and others in the West. Any geologist familiar with Texas and the West will probably agree that Texas geologic problems are the most difficult. Although both terranes are characterized by a large scale uniformity in stratigraphy, the rock exposures in Texas are scarcer and less connected than in the Rocky Mountains, Great Basin and Great Plains regions. Any geologist worth his salt is certain to have his interest stimulated by the variety and scenery of the West, but all of Texas east of the Pecos River is one of the world's most monotonous tracts and withal possessed of perhaps the worst of all climates and environments for the geologist. Discouragement appears to have been the ordinary diet of Dumble and his associates of the Geological Survey of Texas and they must have thrived on it or their immense output of work of lasting worth is no criterion.

Dumble was, with a possible single exception, the first to establish a geologic department for an oil company. He was organizer and manager from the beginning of all the Southern Pacific Oil Companies, the most important of which have been the Pacific, Associated, East Coast and Rio Bravo companies. His activities as an economic geologist extended from South America to China, by way of Alaska. The territory under his immediate supervision embraced the States of Oregon, California, Nevada, Arizona, New Mexico, Texas and Louisiana as well as northwest and northeast Mexico. It is doubtful if there is any natural resource which he was not required to study and to judge. His advice when followed proved very seldom wrong and very generally should have been heeded when it was not.

There was nothing of the spectacular flashiness of that present day incubus, the "go-getter," in Dumble. Always unassumingly modest, quiet, gentle and just he pursued the even tenor of his way and won the results desired without ever making any great ado about them. He had scant respect for half-baked deductions and sloppy work. He insisted that ample time and effort be spent in search of the facts and freely spent money for work of a purely scientific nature, regarded by rival companies as either unnecessary or inadvisable. This policy undoubtedly contributed to his almost unique success. Mr. Dumble's companies had few secrets and his offices in Houston and San Francisco have always served as clearing houses of information and discussion for all geologists interested in the West and Southwest. He firmly believed in the fullest possible cooperation among all engaged in the same line of endeavor. He not only freely published results which other companies would consider as private property to be jealously guarded but encouraged his subordinates to do likewise. His intelligence was sufficiently great to realize that an open, free and fair policy will always ultimately pay large dividends.

It would have been scarcely possible for any one to have commanded more respect, loyalty and sincere personal esteem from subordinates and associates. Many have remained with his organizations when they might have bettered their financial condition elsewhere; others have preferred to return after going elsewhere. Almost without exception those who did leave remained ever his friends. Colonel Newcome always carried with him Shakespeare and Don Quixote because he wished to be ever in company of gentlemen. Everyone associated with Edwin Theodore Dumble knew he was in the company of a gentleman.

Charles Laurence Baker

Houston, Texas

SCIENTIFIC EVENTS

BENACHEION PHYTOPATHOLOGICAL INSTITUTE

A letter was received about six months ago from Mr. Emanuel Benachis, a wealthy resident and ex-mayor of Athens, Greece, in which he announced that the Phytopathological Institute that was being built through his generous gift of ten million drachmas was expected to be ready for work in the summer of 1926. Mr. Benachis outlined in his letter some of the objects of this institute. These may be of interest to plant pathologists, as they are concerned with problems of applied as well as theoretical botany. This brief account of the institute may serve to assist those phytopathologists who intend to visit that part of the world in locating the place and to acquaint all those interested in the phytopathological problems of Mediterranean countries with the salient features of this new institute.

The institute is located on a very beautiful site at Strophylion, a suburb of Athens, and occupies a very extensive area of land. There are a number of buildings devoted to distinct types of work. In the main building are housed the various laboratories. In the warehouse building are stored articles, such as chemicals, glassware, machinery, etc. There is an adminis-

tration building where the various offices and the library are located. Besides the above, there are a number of other buildings serving different purposes, such as insectaries, greenhouses and dwellings for the laborers. The insect collections of the institute are representative of the insect fauna of those regions. The same may be said for the mycological and other collections. The laboratories are well equipped with apparatus for general as well as specialized work in plant pathology and other allied botanical sciences.

Some of the problems upon which the institute has concentrated most of its attention are:

(1) A general survey of the various diseases of the most important crops of Greece. This survey will include diseases caused by parasites belonging either to the plant or animal kingdom, such as viruses, bacteria, fungi, higher plants, nematodes, insects and other animals.

(2) Determination of the meteorological and edaphic factors associated with the development and inhibition of such diseases.

(3) Development of resistant plant varieties by breeding or by introduction from other countries.

(4) Introduction of predaceous insects and nematodes and other such natural enemies for the control of the pests of the cultivated plants.

(5) Preparation, testing and distribution of suitable fungicides and insecticides.

(6) Popularization of phytopathological knowledge among the farmers by lectures and demonstration work.

The thing that was called to my attention in the letter particularly, and which I also want to emphasize in this connection, is the inadequate supply of scientific literature in the library. The founder and staff of the institute will greatly appreciate the efforts of all phytopathologists and entomologists throughout the world in helping them to make up for deficiencies in the pathological and entomological literature. Plant pathologists and entomologists wishing to do so may either send reprints of their publications or ask the librarians of their respective institutions to enter the name of the institute in the mailing list. The address of the institute is: Benacheion Phytopathological Institute, Athens, Greece.

C. P. Sideris

University of Hawaii

OPENING OF THE INSTITUTE OF OPTICS IN PARIS

The Institute of Optics, Paris, of which Dr. Charles Fabry is director, was formally opened on March 17. The opening was attended by the president of the French Republic, Monsieur Doumergue, the minister of public instruction, Monsieur Herriot, and a large number of persons eminent in the political and scientific worlds of Paris. Among the three or four speeches those of the director and of Monsieur Herriot were notable. This institute grew out of an endeavor during war time to care for the design and construction of precise optical instruments. The institute was founded largely through the efforts of the Duc de Grammont, Professor Fabry and a few others in 1919 in a hired building. On March 17 the new and very adequate building was inaugurated, one towards which private funds, including some subscriptions from individuals in America, a portion of receipts of the Pasteur Day in 1924 and substantial contributions not only from French industrials but from governmental funds through the Ministries of Public Instruction and of Public Works, were forthcoming.

The institute consists of three subdivisions, covering fields of scientific research, not only in geometric optics, but in physical optics, a large testing laboratory equivalent to a section of the Bureau of Standards and a school for apprentices.

The founding of this institute marks a coming together of the large industrials and the most competent of the war scientists in their own field.

THE ORGANIZATION OF BRITISH SCIENTIFIC WORKERS

An appeal has been sent from the National Union of Scientific Workers to all professionally qualified men of science and technicians in England, with the object of obtaining their views "on the possibility and desirability of building up a body fully representative of their broader interests." The appeal bears the signatures, with many others, of:

Dr. E. F. Armstrong, Sir William Bragg, Professor F. G. Donnan, Sir Richard Gregory, Sir Robert Hadfield, Lord Haldane, Sir Thomas Holland, Sir F. Gowland Hopkins, Sir Charles Parsons, Sir Horace Plunkett, Sir Humphrey Rolleston, Sir Charles Sherrington, Sir Arthur Shipley and Mr. H. G. Wells.

Following is the text of the appeal:

The National Union of Scientific Workers was founded in 1918 (1) to promote the cause of science in our national life, and (2) to improve the status of the scientific worker. The union, during its eight years of life, has a number of achievements to its credit, but it has not succeeded in becoming what its supporters hoped for —an organization fully representative of the general body of qualified scientific workers. Its membership is still a little below 1,000, although there must be nearly 10,000 persons in Great Britain qualified for admission. We believe that the establishment of such a representative body would be of the greatest importance to science and to those who have made science their profession; we are, therefore, sending this appeal to all qualified scien-

tific workers in the country, in order that their attitude in this matter may be definitely ascertained.

We are aware that other organizations exist in the interests of general science; but they have other functions to perform. The British Association and the British Science Guild, for example, serve the useful function of disseminating the results of research work, and of creating an interest in science among the general public. The Royal Society exercises a dominant influence in scientific research, and appropriately is looked upon as the principal adviser of government; but, being limited to a membership of approximately 450 fellows, it can not fully represent the interests of the general body of scientific workers. There are also various institutions which act as qualifying bodies for certain branches of science; but these are professedly sectional.

A body like the union does not aim to influence scientific work except by promoting and protecting the general interests of the profession as a whole. We consider it should be entirely non-political, but with definite economic and cultural aims—namely, to improve the status of men and women of science, and to aid the cause, both nationally and internationally, of science itself, both pure and applied. The union has already been markedly successful in bringing about improvements in the conditions of service of groups of scientific workers in government and private employ. It has been represented on, or given evidence before, various committees set up by government, when the interests of scientific workers have been involved. On several occasions it has intervened successfully to prevent the shutting down or curtailment of the activities of research institutions supported by public funds. The general secretary, as a member of the East Africa Parliamentary Commission appointed in 1924, was able to stress the importance of scientific research as the basis of economic development. The prominence given to the recommendations contained in the report of this commission are a fitting preliminary to the remarkable report on scientific research presented by Lord Balfour to the Imperial Conference.

These activities must be extended. If the union could speak for a united profession, it would be more successful in obtaining representation for scientific workers on all bodies set up to consider problems of national, Imperial, and international importance. It could make a comprehensive survey of the whole field of research and report on the types of fundamental research likely to lead in the near future to important advances in knowledge and the means most likely to promote them. It could play a valuable part by examining and criticizing the activities of government departments and other organizations concerned with the encouragement and application of science. It could draw up a code of professional ethics. It could make a complete register of all those engaged in work demanding for its performance an adequate training in science. In all these ways the union could undoubtedly become of the greatest scientific and national importance, without trespassing in any way on the functions of existing organizations. Scientific men and women must themselves be responsible for deciding what they consider to be best in the interests of their profession; but we suggest that on the existing foundation of the union can be built up an organization that may be of real service. The name, the precise function, the organization, and rate of subscription of any such body is entirely within the control of its members.

Possibly one misconception that seems to be existent should be removed: some non-members still ask whether to join the National Union of Scientific Workers would not mean being called out on strike in certain situations. The strike is not a possible weapon for scientific workers and the union has never imagined the possibility of its employment.

The present rate of subscription to the union is 30s. per annum; and this may account for the reluctance of many of the younger members of the profession to join; but, with any considerable addition to the membership, it should be possible to reduce the subscription substantially, and we aim at an organization that can be worked with a membership rate of approximately 10s.

In order that we may know what line of action is considered by the majority of scientific workers to be in the best interests of science, we beg you to return the enclosed form duly filled up, whether the response be favorable or unfavorable to the union. It is understood that your reply will not be regarded as an application to join the present or the reorganized society. We wish merely to ascertain your views in order that proposals may be drafted in accordance with the general views of scientific workers.

ANNUAL MEETING OF THE AMERICAN GEOPHYSICAL UNION

THE eighth annual general assembly of the American Geophysical Union will be held on Friday, April 29. Section meetings of the union will take place on Thursday and Friday, April 28 and 29. The general assembly will be held at 2:30 P. M. in the lecture room of the building of the National Academy of Sciences and the National Research Council in Washington. The presiding officers will be H. S. Washington, *chairman,* and G. W. Littlehales, *vice-chairman.* This meeting will include reports by the chairmen of the six sections of the union and a symposium and discussion on "Some Factors of Climatic Control," consisting of seven papers.

The meeting of the section of geodesy will be held on April 28, beginning at 9:30 A. M., with William Bowie, *chairman,* and F. E. Wright, *vice-chairman,* as presiding officers. On the following morning at 9:30 the section of seismology will meet, the presiding officers of which will be L. H. Adams, *chairman,* and N. H. Heck, *vice-chairman.* Among other scientific papers the program includes a series of reports of progress in seismological work in the United States.

The section of meteorology meets on April 29 at

0 A. M., under the chairmanship of H. H. Kimball, G. W. Littlehales as *vice-chairman.* A feature he program is a symposium and discussion on the ls and possibilities of measurements of ultra-violet t in solar spectrum and of the ozone content of the atmosphere. The sessions of the section of terrial magnetism and electricity will be held on il 29, beginning at 9: 30 A. M., with the following iding officers: N. H. Heck, *chairman,* and J. H. linger, *vice-chairman.* The scientific program cons of a symposium and discussion with nine titles the correlations of various radio phenomena with r and terrestrial magnetic and electrical activities. section of oceanography, of which the presiding cers are T. Wayland Vaughan, *chairman,* and G. Rude, *vice-chairman,* meets on April 28 at 2: 30 M. The scientific program consists of nine papers various phases of oceanography. A subscription ner for all interested in oceanography is planned 7 P. M. at the Cosmos Club, after which the last ee papers of the program will be presented, folred by general discussion of oceanographic plans d procedures. The section of volcanology will meet ultaneously with the section of oceanography, with A. Jaggar, Jr., *chairman,* and F. E. Wright, *viceirman,* presiding officers. There are seven sciene papers listed on the program of this section.

An exhibit of geophysical instruments, researches d applications will be displayed during 9 A. M. to P. M., Friday, April 22, through Friday, April 29, 27 (except Sunday, April 24), in the southwest and rtheast exhibit rooms of the National Academy of iences and National Research Council building. The hibits will be arranged for by the executive commits of the different sections and furnished by the trophysical Observatory of the Smithsonian Instiion, the Bureau of Standards, the Carnegie Institun of Washington, the Coast and Geodetic Survey, e Naval Observatory, the Scripps Institution of eanography, the Weather Bureau and others.

SCIENTIFIC NOTES AND NEWS

Dr. Erwin F. Smith, pathologist in charge of the boratory of plant pathology in the U. S. Bureau of ant Industry, died on April 6, aged seventy-three ars.

The three vice-presidents of the American Philophical Society will, owing to the death of the president, Dr. Charles D. Walcott, preside in turn at the eeting celebrating the two-hundredth anniversary of e society to be held in Philadelphia at the end of pril. The vice-presidents are Dr. Henry Fairfield sborn, president of the American Museum of Natal History; Dr. W. W. Campbell, president of the University of California, and Dr. F. X. Dercum, professor of nervous and mental diseases at Jefferson Medical College. Dr. Osborn will give the address at the reception to be held in the building of the Historic Society of Pennsylvania. Dr. Albrecht Penck, professor of geography in the University of Berlin, will be a speaker at one of the general sessions.

Professor W. W. Lepeschkin, of the Charles University, Prague, Czecho-Slovakia, has been appointed visiting plant physiologist to the Missouri Botanical Garden and visiting professor of plant physiology at Washington University for the first part of the college year 1927–28. Professor Lepeschkin will give a course on the physiological processes of the plant from the physico-chemical point of view and will also give three conferences a week to graduate students. Further additions to the permanent staff of the Shaw School of Botany are to be made, but the coming of Dr. Lepeschkin inaugurates a system of annual visiting professorships in botany which will be continued indefinitely.

Dr. Hermann Weyl, professor of mathematics at the Institute of Technology in Zurich, will spend the academic year 1927–28 at Columbia University.

The Royal Academy of Sciences of Holland has awarded the Lorentz medal for the most distinguished work in the field of physics to Dr. Max Planck, professor of physics at the University of Berlin.

Dr. Albrecht Penck and Dr. Hellman, professors at the University of Berlin, have been elected honorary members of the Geographical Society of Madrid.

The council of the Institution of Mining and Metallurgy has, according to *Nature,* made the following awards: The gold medal of the institution to Professor William Frecheville, in recognition of his services to the mining industry and to mining engineering education during a long and distinguished professional career; The Consolidated Gold Fields of South Africa, Ltd., gold medal and premium to Dr. Sydney W. Smith, for his paper, embodying much original research, on "Liquation in Molten Alloys and its possible Geological Significance."

Dr. Hugh H. Young, of the Johns Hopkins University, has been elected an honorary member of the German Society of Urology. Other honorary members elected are: Professors Alessandri, of Rome; Fedoroff, of Leningrad; Ferra, of Turin; Brenner, of Linz; Völcher, of Halle, and Wildbolz, of Berne.

Dr. John A. Mandel, professor of chemistry in New York University, University and Bellevue Medical College, has been elected member of the Deutsche Akademie der Naturforscher, of Halle.

Dr. Clarence W. Hudson, professor of civil engineering and head of the department of the Brooklyn Polytechnic Institute, has resigned. Professor Hudson will travel during the coming year, after which he will devote himself to his consulting practice.

Dr. William H. Welch, of the Johns Hopkins University, has been elected honorary president of the American Social Hygiene Association, to succeed the late Charles W. Eliot.

Dr. John Ambrose Fleming, professor of electrical engineering, University College, London, has been elected president of the Victoria Institute (Philosophical Society of Great Britain).

The *British Medical Journal* states that Dr. Max Rubner, professor of physiology in the University of Berlin, recently resigned in accordance with the age limit of sixty-eight years fixed by the new federal constitution of Germany.

Dr. Emil Abderhalden, professor of physiology at the University of Halle, celebrated his fiftieth birthday on March 12.

Dr. Reinhard Dohrn, director of the Zoological Station at Naples, reports that forty scientific men are now working at the station, including the following from the United States: Dr. R. S. Detwiler, of Cambridge; Dr. J. Graham Edwards, of Baltimore; Miss E. Spencer, of Newark, and Professor Charles Zeleny, of Urbana. The list also includes the names of twenty biologists who are expected to be in residence during the coming year, including Professor Ross G. Harrison, of Yale University, from October 27 to April 28, and Dr. H. Plough, of Amherst College, from November 27 to March 28.

Two foreign geologists, Professor Leon W. Collet, of the University of Geneva, and Dr. E. B. Bailey, of the Scottish Geological Survey, will take part in the geological excursion on wheels, under the auspices of Princeton University.

Dr. Augustus Trowbridge, director for Europe of physical and biological sciences for the International Education Board, will substitute for Dr. Vernon Kellogg, who is unable to attend, at the forthcoming meeting of the board of directors of the Institute of Intellectual Cooperation of the League of Nations.

Dr. J. J. Willaman, of the division of agricultural biochemistry of the University of Minnesota, who has this year received appointment to the International Education Board fellowship, will leave in September for London to make a year's study on the action of fungus enzymes on the protopectin of host plants.

Nils A. Olsen, assistant chief of the U. S. Bureau of Agricultural Economics, will represent this department at the Pan-American Conference on Education, Rehabilitation, Reclamation and Recreation to be held in Honolulu, Hawaii, from April 11 to 16.

Professor E. B. Wilson, professor of vital statistics at the Harvard University School of Public Health, addressed the scientific staff of the Rockefeller Institute for Medical Research on April 8 on "What is Statistics?"

Dr. William J. Mayo, Rochester, delivered the D. C. Balfour lecture in surgery at the University of Toronto on April 5 on "The Relation of the Basic Medical Sciences to Surgery."

Professor Edwin O. Jordan, head of the department of hygiene and bacteriology, University of Chicago, has accepted an invitation to deliver the Gordon Bell Memorial lecture before the Winnipeg Medical Society, Winnipeg, Manitoba, on April 22. His subject will be "Food Poisoning."

Dr. Ralph H. Brown, instructor of geography in the University of Colorado, delivered the initiation address of the Colorado University chapter of Sigma Xi on March 31. The paper was entitled: "Geography and Human Affairs."

Dr. A. Vibert Douglas, lecturer in astrophysics at McGill University, has given an address before the Royal Astronomical Society of Canada, Ottawa branch, on "Sir Isaac Newton and his Influence on Modern Astronomy."

Professor Johannes Walther, of the University of Halle, at present Speyer visiting professor at the Johns Hopkins University, gave a lecture before a joint meeting of the American Museum of Natural History and the New York Academy of Sciences at the museum on April 8. Dr. Walther spoke on "The Gold-Bearing Deserts of Western Australia."

Professor Henri Fredericq, University of Liége, Belgium, gave a Herter lecture at the Johns Hopkins School of Hygiene and Public Health, on March 28, on "Humoral Transmission of Nervous Action."

Sir Oliver Lodge gave an address on "A Century's Progress in Physics" in connection with the centenary celebrations at University College, London, on March 14.

The two-hundredth anniversary of the death of Sir Isaac Newton was observed at Cornell University by a public meeting held by Sigma Xi, at which Dr. William F. G. Swann, professor of physics and director of the Sloane Laboratory at Yale University, was the speaker.

A portrait of the late Sutherland Simpson, who was professor of physiology in Cornell University

inted by Professor Christian Midjo, was presented the university by Dr. Simpson's graduate students April 13. The exercises included an appreciation f Dr. Simpson by his colleague, Dr. James B. Sumer, and a talk on Dr. Simpson as a teacher and vestigator by his colleague in the University of dinburgh, Dr. John Tait, now professor of physiogy in McGill University.

DR. WALKER BELKNAP JAMES, formerly professor f clinical medicine in the College of Physicians and urgeons of Columbia University, died on April 6, ged sixty-nine years.

DR. ROBERT F. WIER, emeritus professor of surery in the College of Physicians and Surgeons of olumbia University, died on April 6, in his ninetieth ear.

THE REVEREND WILLIAM F. RIGGE, for many years stronomer of Creighton University, Omaha, died on arch 31 in the seventy-first year of his age.

DR. CHARLES R. DRYER, professor of geography nd geology at the Indiana State Normal School, uthor of several books on geography, died on March 1, aged seventy-six years.

WM. LOCHHEAD, professor emeritus of entomology nd zoology in Macdonald College, McGill University, ied on March 26 in his sixty-third year.

DR. KARL GRAEBE, formerly professor of chemistry t the University of Frankfurt, recently died at the ge of eighty-six years.

THE annual meeting of the American section of the nternational Union of Scientific Radiotelegraphy is o be held on April 21. The meeting will be at 10:30 . M. in the building of the National Research Counil, Washington, D. C. A number of papers on scienfic radio subjects will be presented. The principal eature of the meeting will be the presentation of reorts of the following technical committees: Methods f measurement and standards, J. H. Dellinger; adio-wave transmission phenomena, L. W. Austin; ariations of radio-wave direction, G. Breit; wave henomena above 3,000 kilocycles, A. H. Taylor; tmospheric disturbances, H. T. Friis.

THE annual meeting of the Society for Experimenal Biology and Medicine will be held at the College of he City of New York on April 20 at 5 o'clock. After he scientific meeting a dinner will be given in the aculty dining-room at which the reports of elections, eports of the secretary and treasurer and other busiess of the society will be transacted.

THE aeronautic division of the American Society of Mechanical Engineers has announced that the Daniel Guggenheim safe aircraft competition will be discussed at their aeronautic meeting at the Hotel Statler, Buffalo, on April 25 and 26. Harry F. Guggenheim, president of the Daniel Guggenheim Fund for the Promotion of Aeronautics, Inc., will deliver a paper at the Monday morning session which will open the discussion.

THE principal speaker at the annual Industrial Conference to be held by the school of engineering at the Pennsylvania State College, May 13 and 14, will be Charles M. Schwab, who for many years has been a trustee of the college. A similar conference is called each spring by Dean R. L. Sackett for the purpose of discussing problems of mutual benefit to the industries of the state and nation and to the engineering faculty. This year the main subject will be the selection, placement and development of technically trained college graduates.

A CONFERENCE to consider resources, generation, transmission and utilization of power in Iowa was held on March 30 and 31 at the University of Iowa with the cooperation of the college of applied science and the extension division.

THE fourth annual meeting of the Alabama Academy of Science was held in Birmingham on April 8 and 9, under the presidency of Stewart J. Lloyd, of the University of Alabama.

THE next meeting of the International Astronomical Union will be held at Leiden, Holland, commencing July 5, 1928.

A MEETING of the French Association for the Advancement of Science is to be held in Algiers from April 13 to 16.

THE French Association of Anatomists has arranged to hold its meeting in London on April 11 to 13 in association with the Anatomical Society of Great Britain and Ireland. The meeting was to be held at University College and about 150 members of the two societies were expected. As the French society includes anatomists from Belgium, Italy, Holland, Switzerland, Spain, Portugal, Poland and Czecho-Slovakia, the congress promised to be of an international character, the first of its kind held in Great Britain.

THE 1927 annual summer field meeting and tour will be held in northern Ohio about the middle of August and will be a study of vegetable and small fruit diseases. The tentative plan is to assemble at the Ohio Agricultural Experiment Station, at Wooster, and to spend half a day there studying the experimental work on vegetable and small fruit diseases. The tour arranged will include the extensive muck section around Lodi and Celeryville. These mucks have been intensively cropped for more than twenty years and diseases are numerous and

severe, especially on celery, onions and cabbage. It is planned to proceed from these sections, northwestward into the general vegetable section where diseases of tomatoes, potatoes, pickles, sweet corn, peas and possibly sugar beets will be studied. The section includes an extensive canning industry. The tour will then continue along the lake and Catawba Island through to east of Cleveland. In this lake section there will be an excellent opportunity to study a wide range of vegetable and small fruit diseases. Numerous experiments will be undertaken in various sections. It will also be possible to visit Ohio's larger general nurseries and one of the largest apple orchards east of the Rockies. The tour will begin at Wooster and end at Painesville, will cover a distance of about 275 miles and will require about three days' time. The meeting is in charge of the Ohio pathologists, with Dr. H. C. Young, of the experiment station at Wooster, acting as chairman. Later announcements will give the details of the trip.

At a recent meeting of the graduate school committee of the U. S. Department of Agriculture, the matter of scheduling a series of lectures by prominent members of the department's staff was discussed. The purpose of these lectures would be to better acquaint the department personnel with important phases of the work being done by the several bureaus. A subcommittee, composed of Dr. C. W. Warburton, director of extension work, and Dr. W. J. Humphreys, senior meteorologist of the Weather Bureau, arranged for the presentation of two lectures, with the idea that if sufficient interest were evidenced a series of lectures would be scheduled for next winter. The first of these two lectures was an illustrated one by Dr. L. O. Howard, chief of the Bureau of Entomology, in the auditorium of the New National Museum in Washington on the evening of March 29. Dr. Howard's subject was "Fifty Years of Economic Entomology."

Dr. Charles Sprague Sargent, late director of the Arnold Arboretum, has bequeathed $10,000 to Harvard College, to be invested and the income to be added to the principal for a 100 years. At the expiration of that period half of the accumulated fund is to be used for the care and maintenance of the arboretum. The income of the other half is to be added to the principal for a further 100 years, after which the income of the total will be available for the use of the arboretum. Dr. Sargent also bequeathed $20,000 to the college, the income to be used to purchase books relative to botany and forestry for the arboretum library.

According to the *Revue Scientifique* the International Education Board has placed a fund at the disposal of Professor Carl Störmer to aid in the erection of a polar observatory near Tromsoë, Norway. This observatory is to be especially designed to study the aurora borealis and similar phenomena. It is to be open to observers from all nations.

The Rockefeller Foundation has given the department of psychiatry of the Kaiser Wilhelm Institute at Munich, $75,000 for their new buildings. This is in addition to $250,000 which has been previously given the institute by the foundation.

Pope Pius has personally donated funds to the Pontifical Academy of Catholic Religion for the creation of an annual prize of 5,000 lire (about $250) to be awarded for research or an original scientific work. The subject for the competition will be chosen by a different academy each year. That for 1928 will be selected by the Academy of St. Thomas Aquinas.

It is announced that the Cincinnati Astronomical Society will erect an observatory on a hill overlooking Big Miami River, six miles west of Cheviot.

According to *Museum News,* the Santa Barbara Museum of Natural History expects to open its new wing, which is being erected in memory of Clinton B. Hale, by May 1 of this year. The wing will include a main room, ending in an apse, in which will be placed a memorial tablet; a herbarium of native plants, a botanical library and work rooms. Work on the new wing was begun on February 8, not long after Mrs. Clinton B. Hale had given the $10,000 to the museum.

The division of plants of the U. S. National Museum has received a valuable collection of plants, chiefly trees, from Professor Samuel J. Record, of the Yale School of Forestry. The larger part of the collection was made by Professor Record during a recent investigation of the Atlantic forests of Guatemala and Honduras. The forest flora of Honduras is practically unknown, and this collection has shown the presence of several new species, a genus new to the North American flora and many extensions of range. Professor Record forwarded also a collection of trees made by employees of the United Fruit Company about Almirante, Panama, and this likewise has proved rich in rare or undescribed species.

Edmund C. Hill, of Trenton, N. J., has presented to the New Jersey State Museum his private mineral and geological collection, according to an announcement by the department of conservation and development. The collection contains many specimens of minerals, rocks and fossils from all parts of the world.

Arrangements have been made with the Ross Tropical Institute and Hospital, of London, England, whereby an exchange of students will be made between the institute and the department of tropical medicine

at Tulane University, New Orleans. Students from Tulane who wish to carry on research in certain fields of tropical medicine will be sent to the Ross Institute and research workers from there will come to Tulane. The arrangement was made by Professor Aldo Castellani, who is honorary director of the Ross Institute and head of the department of tropical medicine at Tulane.

UNIVERSITY AND EDUCATIONAL NOTES

GEORGE PARMLY DAY, treasurer of Yale University, has announced that although the date set for the opening of the $20,000,000 endowment fund drive was still three weeks away, gifts and pledges to the university have already reached a total of $9,500,000.

GIFTS and bequests amounting to $206,841.38 have been received by New York University in the last eight months.

BY the will of Judge Madison W. Beacom, his entire estate was left to Oberlin College. It is estimated that the estate is approximately $75,000.

THE University of St. Andrew's, Scotland, has received from an anonymous donor the sum of £100,000.

PROFESSOR RALPH H. CURTISS has been made director of the observatory and chairman of the department of astronomy in the University of Michigan, in succession to the late Professor William J. Hussey. Professor Curtiss has been assistant director of the observatory of the University of Michigan since 1911 and in charge of astrophysical research since 1907.

DR. LEONARD CARMICHAEL, assistant professor of psychology at Princeton University, has been appointed associate professor of psychology and director of the psychological laboratory at Brown University.

DR. FRANK A. WILDER has been elected to the professorship of geology at Grinnell College. Dr. Wilder was at one time state geologist of Iowa, but for the last twenty years has been president of the Southern Gypsum Company, at North Holston, Va.

ASSOCIATE PROFESSOR ALAN D. CAMPBELL, of the University of Arkansas, has been appointed associate professor of mathematics in Syracuse University.

AT the University of London, Dr. Hamilton Hartridge has been appointed to the university chair of physiology and Mr. W. E. Le Gros Clark to the university chair of anatomy, both tenable at St. Bartholomew's Hospital Medical College.

DR. H. H. WOOLLARD, assistant professor of anatomy and subdean of the faculty of medical sciences at University College, London, has been appointed to the chair of anatomy vacated by Professor Wood Jones at the University of Adelaide.

DISCUSSION AND CORRESPONDENCE

ABOLITION OF THE BUREAU OF CHEMISTRY

IN regard to the notice of a special examination to be held to select a chief of the new Bureau of Chemistry and Soils (SCIENCE, March 4, p. 224), I think the readers of SCIENCE will be interested in knowing why Dr. C. A. Browne, eminent carbohydrate chemist, at present chief of the Bureau of Chemistry, has been legislated out of office.

I am an admirer of Dr. Browne's ability and his splendid career in his profession. I did all I could to persuade him to accept the position of chief of the Bureau of Chemistry at a very considerable financial loss. I will tell the story of this amazing legislation in as few words as possible. The Bureau of Chemistry is the legitimate successor of the first scientific profession named in the organic act establishing the Department of Agriculture in 1862. It is, therefore, the oldest scientific bureau of the department. The Bureau of Chemistry was extremely active in the agitation beginning in 1883 looking to the enactment of the Food and Drugs Law. It was charged specifically by Congress with the duty of enforcing that law. I can not enter into the discussion of the motives which provides for the abolition of the Bureau of Chemistry at midnight on June 30, 1927. I can only tell how this legislation was secured.

Evidently all the principal officials of the Department of Agriculture were in sympathy with this movement. There was a right and a wrong way of doing it. In my humble opinion a bureau which had rendered the eminent services to this country such as the Bureau of Chemistry has done should have had at least some consideration before being led to the guillotine. Nevertheless the program of this execution was prepared with more or less secrecy. There was no noise made about it. The scheme was hatched in the Budget Bureau with the full approval of the high officials of the department. It was submitted to the House of Representatives, with the budget estimate for the Department of Agriculture. There is a rule which reads that no new legislation can legally be placed in an appropriation bill if a single member of the House of Representatives objects to it on a point of order. The high officials of the Department of Agriculture are all aware of this rule. This proposed legislation repealed one of the fundamental parts of the Food Law by abolishing the bureau which Congress had charged with its enforcement. Approximately two thirds of the appropriations for the Bureau of Chemistry were used in the enforcement of this act. This part of the bureau was bodily moved over to a new unit which is

designated in the proposed legislation as "The Food and Drugs Administration." The gamble taken by the authorities was successful. Not a single member of the House raised a point of order. They passed an Enabling Act, transferring the administration of the law to this new unit and abolishing the Bureau of Chemistry absolutely. The mangled remains of the Bureau, *fastigia rerum,* as Virgil would call them, were transferred to the Bureau of Soils, and a new Bureau of Chemistry and Soils was created. This left Dr. Browne high and dry, *sine officio.* The new chief of the bureau, according to the article in Science, is to be handpicked in a kind of examination heretofore unheard of. I have not seen the legislation which authorized the Civil Service Commission to appoint a board of special examiners for this purpose.

The proper way to have gone about this thing would have been the introduction of a bill abolishing the Bureau of Chemistry, establishing a new Bureau of Chemistry and Soils, and creating a new unit of administration for the Food and Drugs Law, with a repeal of that part of the Food Law which charged the Bureau of Chemistry with its enforcement. If this proposition had come before the Congress of 1906, which enacted the Food and Drugs Law, I doubt if it would have received an affirmative vote in either house. Numerous attempts were made during the pending legislation for the law to take the administration away from the Bureau of Chemistry, but every one of these attempts was overwhelmingly negatived. The only persons, then, who really wanted to see the Bureau of Chemistry divorced from the Food and Drugs Act were the adulterators of foods and drugs.

Harvey W. Wiley

Washington, D. C.

10,300,000 VACCINATIONS FOR SMALLPOX WITHOUT ONE SINGLE REPORTED CASE OF SYPHILIS[1]

It has come to the attention of the undersigned that false statements are being circulated, that have caused some people to believe or fear that vaccination against smallpox may cause syphilis. Since the activities under our charge furnish direct evidence in refutation of this idea we have considered it our duty to issue a statement that syphilization as a result of vaccination does not occur.

Before the discovery of smallpox vaccine, the only protection against the dangers of smallpox was by inoculating a person intentionally with the disease and thereby producing, in general, a milder attack than that contracted when smallpox was caught in a natural manner. In this way the inoculation of syphilis along with smallpox, or even of syphilis instead of smallpox, was possible. This possibility also existed when vaccination first supplanted smallpox inoculation, and was performed, as was smallpox inoculation, from the arm of one human subject to another. Cases of syphilis following inoculation or vaccination with human vaccine were, nevertheless, extremely rare. Syphilis, however, is a disease confined in nature to the human species alone, and as soon as the use of calf vaccine instead of human vaccine became universal *the possibility of transferring syphilis by vaccination was entirely done away with.*

Since 1917 the United States Army has vaccinated approximately 4,700,000 members of its personnel; the United States Navy has vaccinated approximately 950,000 members of its personnel; and of these 5,650,000 persons, *not one* of them ever developed syphilis as a result of vaccination. In not one of them was there ever any suspicion of syphilis in connection with vaccination. During this same period, the United States Public Health Service has also vaccinated 2,918,748 persons in carrying out its quarantine, immigration and hospital work. While the service has not always had the opportunity of following up these vaccinations, as is carefully done in the Army and Navy, no one has ever alleged that any particular individual vaccinated by the Public Health Service has contracted syphilis as a result of vaccination.

During the past ten years more than 2,000,000 persons, including school children, have been vaccinated by state and local health authorities in cooperation with the United States Public Health Service, making a grand total of 10,568,748 vaccinations recorded by the government medical services, and not one of the undersigned has ever received an allegation or a statement charging that any particular individual of this number has contracted syphilis as a result of vaccination. In fact, there has never been reported anywhere a case of syphilis attributable to vaccination following the use of bovine smallpox vaccine.

Smallpox vaccine is a standard medicinal product, the quality of which is prescribed by the "United States Pharmacopeia" and as such is subject to the provisions of the pure food and drugs law. Furthermore, smallpox vaccine, together with other vaccines and serums for human use, has been deemed of such importance by the government that its production for sale within the jurisdiction of the United States has been under the special protection of an act passed July 1, 1902, antedating even the pure food and drugs law. Under this law all establishments producing smallpox vaccine for interstate sale must be licensed

[1] The original signed copy of this statement is on file at the Office of the Surgeon General, United States Public Health Service, Washington, D. C.

by the secretary of the treasury upon the recommendation of the United States Public Health Service, and the production is controlled by regulations drawn up by a board composed of the undersigned. These regulations provide for repeated inspections of the producing laboratories, for proper labeling, and for all safeguards which may be thrown about the making of such an important product. At present even the placing of the vaccine in the small tubes and the sealing of these tubes is required to be done in such a way that no hand, even though sterile, touches the vaccine. Repeated examinations of the product, for safety, are required.

This vaccine was used in the vaccination of the millions mentioned in the above table and is exactly the same as that used by doctors in private practice in the vaccination of the general public throughout the United States.

M. W. IRELAND,
Surgeon General, U. S. Army,
E. R. STITT,
Surgeon General, U. S. Navy,
H. S. CUMMING,
Surgeon General, U. S. Public Health Service.

FACTS AND THEORIES IN GEOLOGY

As a member of the American Association for the Advancement of Science for more than one decade, may I be allowed to reply briefly to various people who have expressed themselves adversely regarding my "New Geology, a Text-book for Colleges"?

Professor Edwin Linton's second communication (SCIENCE, Vol. LXIV, No. 1665, pp. 526–7) is the latest of this kind that I have noticed. He looks upon my book as a "transcendent absurdity," though in reality the one point wherein it differs from other text-books on this subject is that it endeavors to make a clear distinction between geological facts and geological theories. Why is not this sharp distinction between facts and theories just as essential for text-books on geology as for text-books on physics or chemistry or astronomy? That I have stated some theories of my own which are not generally accepted is a very small matter; the real peculiarity of my book is that I have endeavored to make this separation, so that the student may have some chance for his intellectual freedom of choice. If I have not always succeeded in making this separation, that would be cause for just criticism; but that this book should try to make this separation hardly entitles it to be called a "transcendent absurdity." I do not think that Bacon or Newton, Linnaeus or Agassiz would look upon it in that light.

Three ideas are outstanding in this text-book and in my various other books:

(1) An emphasis on the fact that uniformitarianism is at best only a theory, to be evaluated according to the facts of modern discoveries, like any other theory.

(2) The fact, as stated by T. H. Huxley, that "All that geology can prove is local order of succession"; and "the moment the geologist has to deal with large areas, or with completely separated deposits," there is danger of "incalculable mischief" in confounding similarity of stratigraphical arrangement with "synchrony" or identity of date; hence that "'not proven and not provable' must be recorded against all the grand hypotheses of the paleontologist respecting the general succession of life on the globe."[1] If this has become a "transcendent absurdity" in this year 1927, I should like to know wherein we have outgrown the "methods" which Huxley condemned in 1862.

(3) That monophyleticism should be frankly and openly repudiated; and we should just as openly and frankly affirm, as Dr. Leo S. Berg, of the University of Leningrad, has done in his recent notable book, that "not only do phyla, classes, and orders not infrequently prove to be polyphyletic, but such is often the case with lesser taxonomic divisions."[2]

As I have been contending for this last idea for many years, it is some satisfaction to see Dr. Berg declaring that "Organisms have developed from tens of thousands of primary forms" (p. 406). *E pur si muove.*

May I call attention to two other works that I have not yet seen noticed in the columns of SCIENCE? One is "The Case against Evolution," by Dr. Geo. Barry O'Toole, issued two years ago by the Macmillan Company. It devotes some twenty pages to endorsing wholeheartedly my geological argument. The other is "The Dogma of Evolution," by Professor Louis T. More, delivered as a series of lectures at Princeton University, in the spring of 1925. This book is issued by the Princeton University Press, and is handled here in England by the Oxford University Press. When works like these are loftily ignored by the official organ of the American Association for the Advancement of Science, is there not danger that we may degenerate into a mere mutual admiration society?

I do not have the space to reply to my other critics, like Arthur M. Miller and Edwin Tenney Brewster. Dr. Chas. Schuchert's professedly formal review of "The New Geology" appeared shortly before I left America. He makes merry over his straw man; for

[1] "Lectures and Lay Sermons," pp. 29, 30, London, 1913.

[2] "Nomogenesis," p. 244, London, 1926.

his "review" is a sheer burlesque of what my book contains. He also complains because I have stolen some of his thunder; in other words, he says I have "appropriated" over two dozen *more* illustrations from his text-book than the few which his publishers authorized me to use. In this Dr. Schuchert is quite mistaken. He seems to forget that I and my publishers may possibly have access to the same original sources for illustrations that he himself had.

Possibly it may interest Dr. Edwin Linton and my other critics to know that the latest example of a "transcendent absurdity," issued by me, is entitled "Evolutionary Geology and the New Catastrophism," and that it was published only a few months ago.

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LONG RANGE WEATHER FORECASTS

IN a review of "Man and Weather," SCIENCE (Vol. LXV, No. 1681, p. 281), March 18, 1927, some personalities may be passed without remark; but the attitude of the reviewer on the problem of long range forecasting should not pass without comment. He holds that such forecasts are not possible at present and by implication that there is little prospect of accomplishment. "No one," he says, "is in position to forecast for California or any other part of the country the distribution of atmospheric pressure even a week ahead, to say nothing of a month or season." Yet he admits "a fair degree of success in seasonal forecasting" in India; and concedes that "we are on the eve of attaining similar success in parts of California."

Years ago this relationship was pointed out in California; and it is our understanding that forecasters on both sides of the Pacific, Okada in Japan, Beals, Bowie and Reed on this side, utilize knowledge of the intensity and extent of the Aleutian infrabar and other pressure distributions in long period forecasts. Across the Atlantic similar procedure is followed. The reviewer has overlooked that in Shaw's "Forecasting Weather," 2nd Edition, p. 181, is a pressure chart on which a forecast *for 14 days was issued* by the Meteorological Office.

Weather maps covering a hemisphere are now available with an increasing number of kite and balloon stations. It is not so difficult now to outline and watch the development of major pressure systems as it once was.

The reviewer will doubtless agree that there is room for improvement in forecasting. The present synoptic map remains substantially the same as fifty years ago. It tells what has happened but not what will happen. If we may not scrap it, we at least should modify it—to tell of the advance of cold-dry and warm-moist fronts, and the interpenetration of strata. It is the conflict of air streams that means accurate anticipating of rain areas and their duration. Winds are initiated by pressure differences, hence the significance of major pressure distributions, controlling the paths and constancy of the fronts. It is gratifying to note a growing appreciation of these points by official bureaus abroad and at home.

ALEXANDER MCADIE

SCIENTIFIC BOOKS

The Insects of Australia and New Zealand. By R. J. TILLYARD, F.R.S., etc. Sydney, Angus and Robertson. 1926. 560 pp.

THE insects of the Antipodes claim our attention for numerous reasons. From Australia came the dreaded Cottony-cushion scale (*Icerya purchasi*), which at one time threatened the destruction of the orange industry of California. From Australia also came the lady-beetles, of diverse species, which have proved invaluable in checking the *Icerya* and other coccid pests. From Australia, Froggatt described the extraordinary archaic giant termite *Mastotermes darwiniensis*, close relatives of which have since been found fossil in Europe. The fauna of New Zealand amazes us by its poverty of types, but it is rich in certain groups. These southern lands have not only furnished many entomological surprises, but they will afford new wonders for many years to come. Nowhere else is there such a good chance for the discovery of relicts of an early fauna, now exterminated in other parts of the world.

In 1907, Mr. W. W. Froggatt, entomologist of New South Wales, published an excellent book of 449 pages, entitled "Australian Insects." In it he gave a readable account of the leading or more conspicuous forms, with very good figures. Those of economic importance were discussed quite fully. Now, after twenty years, Dr. Tillyard gives us a new and more comprehensive book, including also the insects of New Zealand. In this interval, the additions to our knowledge have been very numerous, and very much has been done to arrange and systematize what was known before. Among all the discoveries and additions we must place first the revelation of a wealth of fossil insects of great antiquity, which as elaborated by Tillyard, throw new light on the origin and relationships of the various orders.

Tillyard's book is actually much more than its title might seem to indicate. It is a great contribution to the classification of insects in general, and as such will necessarily be at the elbow of the working entomologist everywhere. We note the extraordinary wealth of

detail, the abundance of figures showing structure, the beautiful colored plates by Mrs. Tillyard, the introduction of the evidence from fossils to elucidate phylogeny. Necessarily, a work of this sort has to be largely a compilation, but few have shown so many evidences of originality. The comparison which comes to mind is with that great classic, Westwood's Modern Classification of Insects. As we remember that when engaged in writing his book Tillyard was during a large part of the time seriously ill, we think of Darwin, and wonder whether ill health is a circumstance favorable to scientific production.

An interesting feature is the census of the Australian and New Zealand species under each family. It can of course only represent existing information. Yet as it stands it brings out most strikingly the great difference between the faunae of the two countries. A few examples will make this clearer. Buprestidae, Aus. 766, N. Z. 2; Mutillidae, Aus. 197, N. Z. 0; Thynnidae, Aus. 438, N. Z. 0; Bombyliidae, Aus. 80, N. Z. 1; Dolichopodidae, Aus. 20, N. Z. 45; Empididae, Aus. 50, N. Z. 110; Tipulidae, Aus. 250, N. Z. 500; Syntomidae, Aus. 52, N. Z. 0; Hesperiidae, Aus. 92, N. Z. 0; Pieridae, Aus. 30, N. Z. 0; Culicidae, Aus. 100, N. Z. 8.

In these statistics, species known to have been introduced by man are omitted. In view of such facts as these, we look with grave doubt upon records of species of bees or other insects, other than strong flying or migratory forms, said to be common to Australia and New Zealand. All such statements should be critically investigated, and it will probably appear that in most cases the determinations were erroneous, in others that the species have been introduced into one of the countries by human means. Sometimes, perhaps, the locality labels will be found to be wrong. Errors are easily made and too faithfully perpetuated by succeeding generations of writers.

A very extraordinary fact is the lack of plant-lice (Aphididae). There are no native species described from Australia; but a single one, apparently native, has recently been described by Laing from New Zealand. The place of the plant-lice is taken by the Psyllidae (Aus. 80, N. Z. 6).

New Zealand is very poor in bees, but Australia extremely rich. Some of the Australian species are minute; Tillyard remarks that *"Euryglossa"* (should be *Euryglossina*) *chalcosoma*, barely 3 mm long, is the smallest of all Australian bees. However, *Turnerella gilberti* is still smaller, only 2.5 mm.

It is a pity that Tillyard did not know Morrison's important paper (1922) on the Maskell genera of Coccidae. Consequently the subfamily Phenacoleachiinae, from New Zealand, is omitted.

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SCIENTIFIC APPARATUS AND LABORATORY METHODS

RAPID DETERMINATION OF SOIL MOISTURE BY ALCOHOL

In the issue of December 31, 1926, of this journal there appeared a brief article proposing alcohol as a very rapid means of determining the moisture content of soils and possibly of some other materials. Since the publication of this paper a great number of letters have been received asking for more detailed information as to the technique, kind of hydrometer used, etc. In view of this large number of inquiries, it has seemed advisable to publish in advance of the main report the directions for executing a moisture determination and other essential information concerning the method.

The alcohol method, as far as it has been investigated, seems to be able to determine the moisture content of soils very rapidly and quite accurately. The rapidity depends somewhat upon the type of soil, which affects the rate of filtering; the time, however, varies from about three to fifteen minutes. In comparison with the oven method the results of the alcohol method run a little lower, not much more than about one per cent. in the heaviest soils. It seems that the alcohol takes out all the water that exists in a physical form. The only kind of water that probably it does not take out is the so-called chemically bound water, and according to the results the magnitude of this form of water is probably not very high. If it is, then it would seem that the alcohol probably extracts some of it, probably the more loosely bound.

For the employment of the alcohol method, as has been worked out thus far, the following apparatus is necessary: (1) alcohol hydrometers made especially for this work. The hydrometers come in a pair. One has a range of from 90 to 100 per cent. alcohol and the other from 80 to 90 per cent. They have a very small volume. They are handled by Eimer and Amend and cost about two dollars apiece. It would be advisable to ask for hydrometers according to the writer's specifications when ordering. (2) An ordinary 25 cc graduated cylinder having an inner diameter of 2 cm. This cylinder is used in measuring the specific gravity of the liquid. (3) An ordinary 100 cc graduated cylinder. (4) A 100 cc funnel. (5) A liter beaker filled with sand. The sand is used to stand in the 25 cc cylinder so that it will be easily adjusted to stand absolutely upright when hydrometer floats. The base of the 25 cc cylinder can be broken off so that the latter can be more easily inserted into the sand. (6) A rod about one half centimeter in diameter. This is used in stirring up soils, such as badly puddled or hardened clays which refuse to slack or crumble easily when coming in contact with

alcohol. The alcohol seems to take out the water very rapidly, probably almost instantaneously, but the alcohol must penetrate or come in contact with all the soil mass.

Before proceeding with the moisture determination the hydrometer is first calibrated. This is accomplished as follows: The specific gravity of pure alcohol, about 96 per cent. by volume, is first ascertained. We will say it is 96 per cent. A volume of 50 cc of this alcohol is carefully measured in the 100 cc cylinder, a 10 cc of water is added to it and the specific gravity is again determined. We will say it is 82 per cent. alcohol. The temperature is also recorded, and the readings are reduced to the same basis, say 20° C. A temperature of 1° makes a difference of about 0.2 per cent. alcohol. For temperatures above 20° C. the corresponding amount is subtracted from the percentage of alcohol indicated, and for temperatures below 20° C. the corresponding amount is added to the percentage of alcohol indicated. When the readings are reduced to the same basis, then the reading of the alcohol which contained the 10 cc of water is subtracted from the reading of the pure alcohol and the difference is divided into the 10 cc of water. This gives the number of cc of water that each degree on the stem is equal to. The standard special hydrometer gives .714 cc of water for each graduation. To find the number of cc of water in the soil sample taken, the difference in specific gravity of the filtrate and the pure alcohol is multiplied by .714.

The general procedure for executing a moisture determination is as follows: Pour 50 cc pure alcohol into the 100 cc cylinder. Add to this alcohol 20 grams of soil whose moisture is to be determined. Disperse the soil by shaking, using one palm as a stopper. Unless a soil is badly puddled and hardened it slacks or crumbles in alcohol. In case of soils which refuse to slack or crumble as in the case of some badly puddled and hardened clays, break them up gently by means of the rod. If clay sticks on the rod, rub latter vigorously on the walls of the cylinder. Soils filter fastest when only shaken and not when dispersed by rubbing. Hard lumps can be gently broken up by a rod without dispersing the soil.

Allow the soil to stand for a minute or two in order that the major portion of soil mass may settle. Then pour supernatant liquid on the filter, allowing filtrate to drain into the 25 cc cylinder which stands in the sand. Only about 12 to 20 cc of filtrate is required. Place hydrometer in the filtrate and take readings. The latter should be taken on straight line to the surface. Take hydrometer out and determine the temperature of the filtrate. Reduce readings to same temperature basis. Subtract reading of filtrate from reading of pure alcohol and multiply differences by .714 or by whatever factor found in calibrating hydrometer, which gives number of cc of water in the sample taken. The percentage of water in the soil is calculated in usual way.

In collecting or preparing a soil sample care must be taken not to puddle it or press it so that the alcohol can penetrate it and slack it easily. Keep the soil as much in its natural crum structure as possible. Care must be taken to rinse vessels with pure alcohol before using.

The waste alcohol can be recovered so it can be used again by treating it with burned lime.

During the process of filtering it is well to cover the funnel to prevent evaporation of the alcohol mixture.

If one has to make many moisture determinations, it would be well to have several cylinders and funnels, so that while one sample is clearing up and being filtered, another is being prepared.

Other forms of alcohol could probably be used equally as effectively as ethyl, but probably this is the most practical.

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SPECIAL ARTICLES

THE ASKENASY DEMONSTRATION OF TRACTION TRANSMITTED THROUGH LIQUID WATER

THAT water and other liquids possess to a great extent the property of cohesion, and that mechanical traction or pull may be transmitted or applied through a mass of liquid in much the same way as through a solid, have long been known, but the experimental demonstration of such transmission still remains outside of the direct experience of most students of natural phenomena. The general concept of taut strands of liquid water and of water masses slightly stretched by traction transmitted through them is familiar enough in the field of plant physiology. Indeed, the molecular phenomena here referred to are so broadly fundamental that they now form the most satisfactory basis for a scientific analysis of the behavior of water and aqueous solutions in ordinary plants. Transmission of traction through liquid water is generally, however, a thing merely to be read about and vaguely, almost mystically, pictured if really envisaged at all. The demonstration of this phenomenon is still regularly omitted from laboratory manuals of plant physiology and general botany, or else the proposed demonstrations are of such nature that they almost always fail, leaving the student to content himself at last with just reading about how

such experiments have succeeded in other hands. Text-books of plant physiology are commonly as unsatisfactory in this connection as are the laboratory manuals. It is notable also that physical and physicochemical treatises generally afford little or no direct help to the perplexed student of this particular corner of the field of molecular physics, while adequate discussion of liquid tension and procedures for its experimental demonstration are not usually included in college and university courses in the physical sciences. Physiology can not here refer to hand-books or courses in the more fundamental fields.

Continuation of such a state of affairs is of course undesirable from the standpoint of plant physiology, since the physical principles here involved constitute a cornerstone in that science. This is true from other standpoints also, for it is not unlikely that the tensile strength of water or that of other liquids may, when more commonly understood and appreciated, become applicable in many fields outside of physiology and in ways as yet unthought of. Capacity to transmit traction is an important characteristic of liquids that has thus far generally escaped conscious appreciation and application in research as well as in the arts and in engineering, although it is known to be inevitably involved in the mechanisms of ordinary plants and may be influential in some hydrodynamic geological processes and in the mechanism of the drying out of some kinds of porous materials. The occurrence of liquid tension in suitable experimental systems and in ordinary plants has been variously demonstrated by a number of experimenters, but the most striking method thus far available for the visual demonstration and study of the transmission of traction through water or aqueous solutions or suspensions is that of Askenasy (1896), which has been modified and improved in detail by Ursprung, Jost and others. In spite of marked improvements, the experiment has remained unsatisfactory, with difficult, awkward or tedious manipulations and uncertain results.

We have been engaged for about a year in further detailed study of this classic experiment, with the hope that additional improvement in technique might at length render it really suitable for elementary lecture and laboratory demonstration and also for the direct utilization of the tensile strength of water in research experimentation wherever this may be desirable. Our attempts have been passably successful, and the phenomena in question can now be readily demonstrated within a few hours, and without undue preliminary work. A brief discussion of our procedure was presented before the physiological section of the Botanical Society of America at its Kansas City meeting in December, 1925, and a description of the method is included, with a diagram, in the new (third) American edition of Palladin's "Plant Physiology" (P. Blakiston's Son and Co., Philadelphia). Still further improvement and simplification, as well as useful applications in research instrumentation, etc., will doubtless be brought forward, but enough has now been accomplished to make possible and feasible the satisfactory introduction of the Askenasy demonstration in laboratory courses or as a lecture experiment in either the physical or physiological sciences.

We shall not describe the experiment in detail in this paper, but we wish to call the attention of science teachers to the pressing need for a more precise and general appreciation of liquid cohesion and its corollaries, particularly the transmission of traction through liquid water as this occurs in ordinary plants. We wish especially to request other experimenters to send to this laboratory information regarding their own experiences with the Askenasy demonstration, whether successful or not. We should like to bring together in a future publication the experiences of all who have been interested in this experiment, with special reference to difficulties and experimental failures as well as successes.

Misunderstanding may exist as to just what constitutes an Askenasy demonstration. A simple form of the apparatus, which cares for all the essentials, consists of a porous porcelain cylinder, closed at one end and attached by a rubber stopper to the upper end of a vertical, small-bore glass tube, a meter or more in length. Tube and cylinder are filled with water without air bubbles of any considerable size, and the lower end of the tube dips into mercury in a reservoir below. Water moves outward and evaporates from the surface of the cylinder but undissolved gas can not enter, for the pores of the cylinder wall are effectively plugged with water, the air-water meniscus within each pore being held fixed by capillary forces capable of withstanding an excess of several atmospheres of external air pressure. Indeed, the entire upper portion of the container that encloses the diminishing water mass (*i.e.*, porcelain cylinder and glass tube) may, for the present purposes, be regarded as practically rigid, like the glass tube of a barometer. Consequently the volume of the water can become adjusted to its diminishing mass only by a rise of the non-rigid boundary between water and mercury at the base of the system. As water continues to evaporate from the water-soaked walls of the cylinder, the water column becomes shorter and the loss is replaced by mercury, which rises in the tube from the reservoir.

At any moment in the progress of the experiment the pressure at any level (calculated as one calculates the pressure in the tube of a barometer and neglect-

ing a very small correction for capillary depression) is the difference between the pressure at the base of the tube and the pressure corresponding to the liquid column extending upward from the base to the level in question. The pressure is thus always least at the top of the system, and greatest at the bottom. At the base of the tube the pressure remains practically unchanged throughout an experiment, being equivalent to the atmospheric pressure acting on the surface of the mercury in the reservoir, increased by a small value to account for the depth of the tube-opening below the mercury surface. But the pressure at any higher level, which is always smaller than that at the base of the tube, becomes still smaller as water lost above is replaced below by the much heavier mercury, and this decrease continues until the mercury-water boundary attains the level in question, after which the pressure in the mercury at that level remains constant with still further elongation of the mercury column. The pressure at the mercury-water boundary becomes continually smaller as this boundary ascends, becoming zero when the column of mercury just balances the opposed external pressure —that is, when the height of the mercury column in the tube just corresponds to the maintained pressure at the base. At this time the pressure at every level in the mercury is positive, being greatest at the bottom and zero at the top of the mercury column, and the pressure at every level in the water is negative, this negative value being of course numerically greatest at the top of the system and zero at the mercury-water boundary. The water is then all in a state of tension; it is taut, like a vertically suspended rope or wire. The water adheres to the nearly rigid glass, rubber and porcelain walls and to the mercury below. As still more water is lost by evaporation from the cylinder and the mercury column continues to elongate upward, the upper portion of this column also passes into a state of tension and we have a demonstration of the transmission of traction through the upper portion of the mercury as well as through all of the water. The taut mercury adheres to the water above and to the very thin water film that intervenes everywhere between mercury and glass and this film adheres to the glass, thus acting as an adhesive cementing mercury and glass together.

The essential pressure relations outlined above may be stated algebraically as follows, all pressure values being expressed as heights of equivalent mercury columns, in centimeters.

$$P = B + b - (H + \frac{h}{13.6} + D)$$

In this expression, P is the pressure at any level in the liquid column, whether in mercury or water. It is clearly negative, and there is tension at the given level, if the expression in the parenthesis is greater than $B + b$. B is the atmospheric pressure on the mercury surface in the reservoir outside of the tube (usually the current barometer reading), while b is the depth to which the tube projects downward into the mercury in the reservoir. H is the vertical length of the mercury column between the base of the tube and the given level, while h is the vertical length of the water column below the given level, between it and the water-mercury boundary below. D is a small value due to capillary depression. (It may be taken as 0.8 cm. for a tube of 1.5 mm. bore.) If the level considered is *below the water-mercury boundary*, then h is zero. If the level considered is *at this boundary*, then h remains zero and H is the total vertical length of the mercury column in the tube, measured from the lower end of the latter. If the given level is at *the top of the system*, then H remains as in the last case and h is the total height of the water column measured from the water-mercury boundary to the top. To illustrate, if B is 75.5 cm., b is 2 cm., H is 125 cm., h is 20 cm., and D is 0.8 cm., then $P = 75.5 + 2 - (125 + 20 + 0.8) = 77.5 - 145.8 = -68.3$ cm. The negative pressure at the given level, which is 143 cm. above the level of the mercury in the reservoir in this case, is equivalent to 68.3 cm. of a mercury column or about nine tenths of an atmosphere. This is a measure of the tension or traction at the given level. (It should be noted that the expression "negative pressure" is sometimes erroneously used to denote simply *decreased positive* pressure, a positive pressure lower than that of the surroundings.)

In an experiment of this kind the liquid column in the system eventually breaks in every case, sometimes in the water (in cylinder or tube) and sometimes in the mercury. When rupture of the column occurs before the pressure at the highest point of the system has become negative, then the experiment is a failure in respect to the demonstration of traction and liquid tension (for none is developed in such a case), although it does successfully demonstrate what has been called the "suction power" of evaporation. The suction developed at any level in the system is then equivalent to the (positive) value of P, as found by means of the equation. It is of course greatest at the top of the system.

An experiment to show the "suction power" of evaporation from a fine-pored membrane is regularly performed in laboratory courses in plant physiology and is generally described and figured in the textbooks of that science. The arrangement is essentially like that of the Askenasy experiment. No difficulties are involved and the mercury column gradually elongates and reaches a height of 60 or even 70 cm.

any cases, but (unless proper precautions have been ccessfully taken to make this really an Askenasy exeriment) the liquid column breaks before the presure at the top has become negative in sign. Every skenasy experiment demonstrates suction before any ension is developed and it demonstrates both suction below) and liquid tension (above) when the presure at the top is negative.

It is this suction experiment that was referred to y Dr. C. A. Arndt (SCIENCE for May 21, 1926, page 27), who seems to have failed to realize that traction nd liquid tension can not begin until after the posibilities of ordinary suction have already been exausted. This author was apparently not dealing ith the Askenasy experiment at all. His "superior esults" are to be taken as bearing upon the suction xperiment only, being consequently just failures for he Askenasy experiment. Without additional data barometric pressure, length of water column above he mercury in the system, and bore of tube) even he "greatest total height" given, 28 inches, is not in self evidence of liquid tension, although the smallness of the difference between this value and the noral barometer reading (about 30 inches for Philadelhia) indicates that the pressure in the top of the ystem was as low as one or two inches of mercury. horter mercury columns of ten or twenty inches, uch as Dr. Arndt mentions, surely represent failures s far as the demonstration of tension by the Askenasy ethod is concerned. From Dr. Arndt's printed tatement and also from correspondence with him it clear that the pressure on the surface of the merury in the reservoir was the current barometric presure.

Plant physiology requires as careful thinking as do he physical sciences, and students of plant water elations should be led to distinguish clearly between ction and traction. In the case of suction the ongating or moving column of liquid is under the ction of two opposing external forces, one larger han the other but both tending to compress the quid and shorten the column. In the case of tracon also there are two external opposing forces, but oth tend to overcome the cohesion of the liquid and retch the column. In the first case the liquid column slightly compressed, in the other it is slightly retched (tension). In suction the liquid is *pushed* p and in traction it is *pulled* up. For a demonstraon of suction alone it is not necessary to exercise ny special care in setting up the apparatus, but pecial treatment is generally necessary if any tension r traction is to be developed.

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SIMPLE SEISMIC MEASUREMENTS

THE measurement of earthquake acceleration maxima by observation of the fall of vertical columns was proposed more than forty years ago. The condition that a properly directed horizontal acceleration should be sufficient to overturn a simple rectangular parallelopiped was stated by Professor C. D. West as $a = gb/h$, where h is the height and b the breadth of the paralellopiped, and g is the gravitational acceleration. This relation results at once if one equates the inertial moment about a lower edge of the parallelopiped to the gravitational moment about the same edge. By observation after a quake of the status of a number of parallelopipeds having different ratios b/h an estimate of the magnitude of the maximum acceleration was to have been obtained. But upon testing this method by experiment Milne[1] and Omori[2] found West's formula to be inapplicable to earthquake-like accelerations. Discrepancies as high as 35 and 40 per cent., some positive and some negative, were recorded.

There seem to have been two reasons for this disagreement. The acceleration of West's formula—or rather any acceleration in excess of it—although undoubtedly sufficient to *start* the overthrow will not bring it to completion if the duration of the acceleration be too brief. Seismic accelerations are not constant accelerations but are of an alternating nature and may rise to maxima much higher than that expressed by the above equation and yet die away so quickly that the complete overthrow does not take place. On the other hand, the alternating character of the acceleration may in some cases result in the upset of the parallelopiped through the development of resonant oscillations, even though West's acceleration is never attained. In this case the elasticity of the paralellopiped and of its foundation play an important part. These errors, though opposite in sense, can not be expected to annul each other, and a discrepant result, difficult at present to predict, will in general remain.

West's equation, in short, does not apply to the case of an object overturned by an acceleration of alternating or oscillatory character because it was never formulated to fit such conditions. It correctly defines the minimum acceleration, however attained, at which the object will start to turn over. But only for special cases, such as that of a constant acceleration, does the formula state the acceleration competent to complete the overthrow.

The theoretical treatment of an object overthrown by simple harmonic motion does not appear to have been presented. Galitzin dismisses the matter with the observation that the problem offers real difficul-

[1] J. Milne, Trans. Seis. Soc. Japan, Vol. 8, 1885.

[2] J. Milne and F. Omori, *Seis. Journ.*, Vol. 1, 1893.

ties. The difficulties are substantially reduced however by the assumption that the stability is such that the object rotates through only a small angle ($\tan \vartheta = \vartheta$) before coming to a position of instability. This assumption fortunately does not impair the application of the results to actual earthquake accelerations up to the intensity ranked by Cancani as "Very disastrous." The geometrical form of the object considered is of no real importance and we may drop all restrictions of this kind and assume only a rigid body stably supported by a horizontal axis somewhat lower than its center of mass, and a brace or stop of some kind which prevents rotation under the action of gravity. The object thus described will be referred to as a bar. The angle made by a vertical and the perpendicular dropped from the center of mass of the bar upon the axis of rotation will be called ϑ.

Now it may be shown that the bar will just be thrown down by a horizontal simple harmonic motion directed at right angles to its axis if the maximum acceleration of the motion has the value

$$a = g\,\vartheta\sqrt{1+\frac{4\pi^2R^2}{g\,L\,T^2}}.$$

In this equation R is the radius of gyration of the bar, L the distance from the center of mass to the axis of rotation and T the period of the oscillation. The derivation of the equation is too lengthy to be appropriately presented here and it must suffice to state that it has been deduced by rigorous methods and completely verified by experiment.

The experimental work was conducted upon an oscillating table capable of horizontal simple harmonic motion of adjustable period and amplitude. Every variable quantity represented in the above equation was varied through wide limits, and of over five hundred observations every one conformed to the equation within the leeway of two or three per cent., which the uncertainties of observation permitted.

It is worth noticing that with an infinitely long period of oscillation—and therefore with constant acceleration—the equation reduces to $a = g\,\vartheta$, which is West's formula stated in a slightly different way.

A series of similar bars of suitable form, mounted at a variety of angles of inclination so that an earthquake would cause some to fall and leave others standing, would yield important information about the nature of the oscillatory horizontal movement. But it is evident from the equation that a knowledge of the critical angle dividing the fallen from the standing bars will not in general suffice to determine the maximum acceleration, since the acceleration is a function both of this angle and of the period of oscillation. The influence of the period can only be suppressed by making the whole second term under the radical sign a small quantity, even for short periods. Experiment shows that this may be done by a suitable choice of the form and dimensions of the bar, so successfully, at least, that for all oscillations with periods greater than one fourth of a second the maximum acceleration is fully determined by the critical angle, with an error which can not be greater than six per cent. and which is much less in nearly all cases. It is probably not worth while at present to strive for greater accuracy at this point, since the errors resulting from the assumption that the earth motion is simple harmonic have not been investigated.

The problem may be approached in another way. Referring again to the equation one sees that a knowledge of the critical angle, as obtained by observing a given set of bars, gives an equation in which a and T alone are unknown. From a second set of bars, different from the first in R or L, another critical angle is observed, giving another relation between a and T. The two equations are independent and together suffice to determine both a and T. With the acceleration and period known the corresponding amplitude is of course readily computed. These determinations are subject to three errors only. The first is the error in determining the critical angles; it may be made as small as desired by the use of a larger number of bars. The second is the error involved in the assumption that ϑ is small; this is entirely negligible for quakes of small intensity and rises only to about one per cent., with very disastrous quakes. The third error is that of the assumption that in the neighborhood of the maximum acceleration the earth motion is simple harmonic. Assumptions of this kind have often been made, but never, so far as known, critically investigated. It is impossible at the present time to make a numerical statement of the exactitude of this assumption, but its validity becomes of less and less importance for present purposes as the radius of gyration of the bars is diminished. It appears probable to the writer that errors of this sort in the determination of maximum acceleration can be kept below five per cent.

It appears possible therefore to make important earthquake measurements with extremely simple and inexpensive apparatus. Through the courteous cooperation of Dr. T. A. Jaggar instruments are soon to be located upon the island of Hawaii, a region which by reason of frequent local quakes should serve admirably as a proving ground for seismometrical devices. A more complete paper upon these subjects will be published in the near future.

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